

fully adapted apparatus must have originated in an organ performing some comparatively simple function. The question at issue may perhaps be stated as follows:—In the cases where the nectar-glands are now well developed has there been a special course of structural development in close relation with the need of the plant for protection? Has there been a course of evolution such as we may believe has taken place in the formation of the food-bodies in *Acacia sphærocephala* and *Cecropia peltata*, or should we not rather believe that the sweet secretion has been developed in connection with some unknown process of nutrition; according to this view, a well developed system of glands may continue merely performing some obscure excretory function, and consequently, although the presence of nectar-glands has undoubtedly been of the utmost importance in determining the survival of certain species, yet it is hardly fair to assume that all nectar glands were originally protective in function. For many plants secrete large quantities of sweet fluid, which serves no such purpose. This argument is given by my father in his "Effects of Cross and Self-Fertilisation" (p. 402). In addition to the facts there given in support of this view, a curious case described by Prof. H. Hoffmann may be mentioned ("Ueber Honigthau," 1876). He states that numerous large drops of sweetish fluid appeared on the under-surface of the young leaves of a camellia. He also alludes to a similar abnormal production of honey-dew on an ivy plant.

In the case of introduced plants, we see how an already existing quality may, without any special course of development, become of vital importance to its possessor. Thus, Mr. Belt shows ("Naturalist in Nicaragua," p. 74) that the lime, *Citrus limonum*, is able to exist in a wild state, because its leaves are, from some unknown reason, distasteful to the leaf-cutting ants; whereas the orange, *C. aurantium*, and the citron, *C. medica*, can only survive with the help of man.

Fritz Müller concludes his letter with some curious facts on kindred subjects:—

"The extreme variability of the nectar-glands on the leaves of many plants, is a somewhat remarkable fact. Thus our *Citharexylon* has normally two large glands at the base of the leaves, but sometimes there is only one, and sometimes none at all; besides these there are small glands scattered over the surface of the leaf, the number of which varies from twenty to none. Similar variations occur in the nectar-glands of *Alchornea erythrospermum*, and of a *Xanthoxylon*. It seems to me probable that in all the cases at present known, these glands serve to attract protecting ants; and I here agree with Delpino, although I do not hold with him that caterpillars are the chief enemies which are guarded against by Pheidole and Crematogaster; but I think with Belt that these latter ants protect the plant against the leaf-cutting species. Indeed it is precisely those plants which are free from the attacks of ants that seem to be especially well fitted for caterpillars. Thus the larvæ of *Gynacria* live on *Cecropia peltata*, those of *Epicalia numilia* on *Alchornea erythrospermum*. On the Cayien (?) whose leaves are furnished with nectar-glands, and are visited by protecting ants, the caterpillars of many species of *Callidryas* are found. Finally, as far as I know, all the larvæ of the genus *Heliconius* feed on *Passiflora*. Moreover, the same relation holds in the case of plants protected in other ways, for instance, by stinging hairs or by poisonous sap. How numerous are the larvæ found on the European stinging-nettle. In this country we find the caterpillars of 'Ageronien' on the stinging *Dalechampia*; and again those of some species of *Danaï*s on *Asclepias*, which is protected by its milky juice."

FRANCIS DARWIN

Down, Beckenham, May 21

Quartzite Implements at Brandon

AT the recent conference held by the Anthropological Society on the present state of the question of the antiquity of man, the president, Mr. John Evans, referred to the finding of implements made of quartzite at Brandon, and remarked that as that rock did not exist in the neighbourhood excepting in the glacial drift, the implements must have been made from pebbles obtained from the glacial beds, and were therefore of post-glacial age. This statement was made at the end of the meeting when there was no opportunity of replying to it, and as its effect must have been great, I shall be obliged if you will allow one who believes that none of the palæolithic implements are of post-glacial manufacture to make some remarks upon it.

For a full description of the implement-bearing deposits near

Brandon I must refer to an able paper by Mr. J. W. Flower in the twenty-fifth volume of the *Quarterly Journal of the Geological Society*. Gravel Hill, near Brandon, is an isolated hill rising to a height of 91 feet above the river, from which it is nearly a mile distant. It is covered with gravel which is mostly, and in some places entirely, composed of quartzite pebbles. Mr. Flower estimates that three-fourths of the whole are of quartzite. In this the gravel differs from that of other pits in the neighbourhood of Brandon, as for instance, that at Bromhill, which contains only one thirtieth part of quartzose pebbles.

At Gravel Hill, along with some hundreds of flint implements, four made of quartzite, similar to that of the pebbles, have been found. All the implements are usually found at the bottom of the gravel, and occasionally lie on the chalk. On the supposition that the quartzose pebbles, from which some of the implements have been made, were brought by ice in the glacial period, some such succession of events as the following must have occurred. 1. Ice, from the north, carried thousands of quartzite stones and deposited them in immense abundance over a limited area. 2. Man afterwards made implements from some of them. 3. The whole of the pebbles were rearranged and formed into beds of gravel with the implements at the bottom, whilst the distinctive character of the deposit was retained.

This ingenious but complicated theory is not necessary, for quartzose boulders and pebbles are found in deposits much older than the glacial period not very far away from the locality, and may exist beneath the drift close to it. It has long been known to geologists that there are many fragments of old crystalline rock in the upper greensand. They have been described by Mr. Bonney in his geology of Cambridge, and very fully by Messrs. Sollas and Jukes-Browne, who state that fragments of gneiss, mica, and hornblende schists, talcose schists, granites, vein quartz, grits, quartzites, and slates are very numerous in this bed. It ranges northward from Cambridge, and is lost beneath the surface gravels and boulder clays, but it is not at all improbable that it may run along to the west of Brandon, and there contain even more quartzose fragments than in Cambridgeshire.

I may remark in conclusion that Mr. Flower, in his description, states, that he is disposed with the French geologists, to ascribe the outspread of the gravels to some powerful cataclysmal action, and that he does not know of any boulder clays in the course of the river from which such a mass of pebbles could have been derived.

THOMAS BELT

Cornwall House, Ealing

The Migration of the Swiss Miocene Flora

WITH reference to the route the plants took which formed the European miocene flora, I should be glad to know *why* Dr. Unger considers it to have been from America to Europe. He says: "There is more than one reason for thinking that the centre from which our lignite flora has sprung was far away from Europe—in the southern parts of the United States" (*Journ. of Bot.*, iii. 17). He thinks that the living flora of that part of America is the lineal descendant of that which gave rise, by aid of "Atlantis," to the Swiss miocene flora. But is enough known of the miocene flora of the United States to infer this? Prof. Heer says that the methods of comparison he employed "incontestably prove that Switzerland was inhabited by types now scattered over every part of the world [agreeing in that respect with the existing Arctic flora], but of which the majority correspond with species of South U.S. of America; the Mediterranean region of Europe ranks second; Asia Minor, the Caucasus, and Japan third; the Atlantic Isles fourth, and North Holland fifth" (*Nat. Hist. Rev.*, 1862, p. 154, quoted by Oliver). Prof. Oliver and Sir Charles Lyell think that the route was by Japan, and not by the Atlantis; but still (Sir Charles, at least) from America to Europe. Heer, in his "Primeval World of Switzerland" (vol. i. p. 325, Eng. ed.), says the *Glyptostrobus heterophyllus* of Japan "has probably been derived from the tertiary species" [of Europe]. Similarly, in comparing the *Taxodium distichum miocenum* with that of America, he observes: "It is very interesting to find that the ancestors of the existing American swamp-cypresses were formerly spread over the whole of Europe, as far as 78° N. lat. Again, of *Sequoia Langsdorffii*, he observes: "It probably formed a zone round the whole earth in high northern latitudes."

Instead, then, of regarding either Switzerland or the South U.S. as a "centre," I would suggest that the miocene flora was uniformly spread over the whole of the regions bordering the

entire Arctic circle, just as the Arctic flora is now; Asia, Japan, and North-West America being then continuous; that as the climate became cooler in the pliocene epoch, it was driven southwards along every meridian, its descendants now existing in the localities above mentioned. It will be seen that these form a belt all round the globe, roughly speaking, between the 30th and 40th parallels of latitude. Migration to some extent might have taken place along that belt, but the great migration I suggest was probably from north to south, and not from east to west, or *vice versa*.

The above theory is simply an adaptation of that given by Dr. Hooker for a similar dispersion of the existing Arctic flora, which replaced the temperate floras of Europe, Asia, and America; but which on the return of a temperate climate retreated northwards as well as up temperate and even tropical mountains, perishing, however, in the low lands.

GEORGE HENSLOW

The Fertilisation of Orchids

WHILE botanising this spring in Portugal, I was struck with the fact that scarcely one of the orchids—species of *Ophrys* principally—that I had collected for my herbarium, or examined in the field, seemed to be fertilised, for none presented the least indication of having had pollen applied to its stigmatic surface; and I examined flowers in every stage of expansion, from the opening of the bud to the withered and shrunk up floral envelopes. Each one, I remarked besides, contained its own pollinia, their caudicles in their respective glands and in their natural position. I was so struck with this, that one day (March 31) I gathered and examined forty-five different flower-heads, and of all these only one was found to have pollen-grains on its stigma, and all, the fertilised one included, had their pollinia intact. The locality was the Tapada d'Ajuda, or Royal Park, situated just outside the city walls of Lisbon, an inclosure containing many acres of land, clothed in spring with a rich flora, and a favourite entomological hunting ground, teeming with Coleoptera, Hymenoptera, and the commoner Lepidoptera. Several of the orchids contained aphides, and a few harboured a species of small red ant.

On April 21 I again made similar observations, selecting the same place, as it was most accessible to me, and because several orchideæ grew there in the greatest profusion. On this occasion I examined over thirty flowers—none of them the same as I had examined in March, for I had plucked these at the time, but there was not one that did not possess its pollinia, and, as on the previous occasion, none of them showed any appearance of having had pollen-grains in contact with their stigmas.

Yet the Tapada, during the spring, produces these orchids by the thousand, vigorous, healthy, sweet-smelling plants, and in sunny days its air is perfectly alive with insect life, as I have said, of every kind.

HENRY O. FORBES

Old Hall, Highgate, N.

New Meteor Radiant

THE "two remarkable meteors" closely following each other, observed by Mr. Hope on May 13 (*NATURE*, vol. xvi. p. 43), proceeded most probably from a shower in the small southern constellation Crater. Your correspondent describes the point of first appearance as a little south of Arcturus. If this means about 7° or 8° below that star, then the observed courses accord well with the centre indicated, which is apparently quite a new radiant. From the Italian catalogue of 7,512 meteor paths (observed by Schiaparelli, Denza, and others in 1872) which I have lately been reducing, I found this shower at R.A. 170°, Dec. 10° S. (near $\epsilon - \theta$ Crateris) for May 3–15 from nine meteors.

Ashley Down, Bristol, May 27

W. F. DENNING

OUR ASTRONOMICAL COLUMN

ANTHELM'S STAR OF 1670.—The small star which is very close to the position of this object, as determined from the observations contained in Lemonnier's "*Histoire Celeste*," deserves to be closely watched, as there is more than a suspicion of its variation within narrow limits. Thus in August, 1872, it was exactly equal to a star which follows it 12' 58". in R.A., 4' 9" to the north; while in November, 1874, it was as certainly fainter by half a magnitude. This small star precedes the variable S.

Vulpeculæ 43' 6s., and is north of it 2' 11"; two other small stars follow less than 2' from the parallel, 22' 4s. and 30' 6s. respectively.

Prof. Schönfeld found the place of Anthelm's star from the observations of Hevelius and Picard,

R.A. 19h. 41m. 37s. N.P.D. 63° 2' 3" for 1855.0.

The place of the suspicious star alluded to above is, for the same epoch,

R.A. 19h. 41m. 43' 5". N.P.D. 63° 2' 7".

It was meridionally observed at Greenwich in 1872.

D'ARREST'S COMET.—As yet we hear nothing of observations of the short-period comet of D'Arrest, for which M. Leveau has worked so laboriously, with the view to facilitate its discovery at the present return. The intensity of light was at a maximum about May 22, but diminishes gradually during the summer. Nevertheless, early in August it is still of the same amount as when Prof. Schmidt discontinued his observations at Athens in December, 1870, at which time he stated he could have followed it longer but for the want of an ephemeris. Subjoined are the comet's calculated positions during the absence of moonlight in the present month, for Paris noon:—

	Right Ascension. h. m. s.	North Polar Distance.	Distance from the Earth.
June 7 ...	1 18 58 ...	85 26' 2 ...	1' 613
„ 9 ...	1 25 14 ...	85 11' 8 ...	—
„ 11 ...	1 31 27 ...	84 58' 2 ...	1' 611
„ 13 ...	1 37 35 ...	84 45' 2 ...	—
„ 15 ...	1 43 40 ...	84 33' 0 ...	1' 609
„ 17 ...	1 49 41 ...	84 21' 5 ...	—
„ 19 ...	1 55 38 ...	84 10' 7 ...	1' 607

Though the comet will not arrive at its least distance from the earth during the present visit (1' 396) until October 20, it passed its perihelion on May 10. The period of revolution is now 2,434 days, or 35½ days longer than at its last appearance in 1870.

THE D'ANGOS COMET OF 1784.—Perhaps no person who has been occupied in astronomical observation and calculation has obtained for himself, rightly or wrongly, a more unenviable notoriety than the Chevalier D'Angos, who, in the latter part of the eighteenth century, was possessed of a small observatory in the island of Malta. From the unusual character of some of his statements his name came to be associated with anything in the way of observation that appeared to be apocryphal, and we find not only Zach was in the habit of terming doubtful assertions "*Angosiades*," but even Pastorf, who himself put upon record more than one suspicious statement, appeared to consider that he was establishing the good faith of an observation of a comet in transit across the sun's disc by declaring that it was not an observation made "*à la D'Angos*." We pass over on this occasion the reported observations by D'Angos in 1784 and 1798 of a comet or planet upon the sun, with the view to presenting the reader with a brief outline of the actual state of a case that has been open to still greater suspicion, viz., his observation and calculation of what appears in some of our catalogues as the second comet of 1784; and we may be pardoned for bringing together here particulars which though probably known to those who have access to a good astronomical library, are not so likely to be within the cognisance of those who cannot command such a collection. And further, it is almost essential to bring the main points in the case into one view, to enable the reader to judge for himself whether D'Angos is deserving of the opprobrium which has been cast upon him or not.

Writing from Malta on April 15, 1784, D'Angos apprised Messier at Paris that he had discovered a comet in Vulpecula on April 11, and he inclosed two approximate positions observed on the mornings of these days. He stated that the comet was very small, without tail, and with only "a slight appearance of nebulosity." Messier